

de M. Steenstrup. Si vous consultez le *Record of Zoological Literature* pour 1869 (vol. vi.), vous y trouverez, p. 139 :—

"*Selache maxima*.—A detailed description and figure of an example from the coast of Portugal is given by M. Capello under the name of *Cetorhinus blainvillii*, *Four. Ac. Sc. Lisbon*, No. vii., p. 233."

Je vous envoie par la poste le No. vii. du *Four. des Sc. de Lisbonne*.
J. N. BARBOSA BOCAJE

Gold in Carboniferous Conglomerate

MANY of your readers are aware that the fact of the occurrence of gold in Lower Carboniferous conglomerate as in New South Wales is not at all new. The Gay's River Gold Field of Nova Scotia, where the gold occurs in Lower Carboniferous conglomerate resting on the edges of Cambrian slates having small veins of auriferous quartz, was first pointed out by Prof. Hartt and elaborated by myself in a paper communicated to the Nova Scotian Institute of Natural Science in 1866. In Dawson's "Accadian Geology," of 1868, the same fact is referred to; also in Seluria, Prof. R. Jones received specimens of the conglomerate from me in Paris, 1867, to satisfy Sir R. J. Murchison of the fact. In the collection of ores and concrete minerals sent by H. S. Poole, Esq., Government Inspector of Mines to the Centennial Exhibition, in my charge, was a very instructive specimen of slate with a little of the conglomerate attached, having a beautiful display of gold. This was much admired. The conglomerate of Gay's River is overlaid by limestone with Lower Carboniferous fauna and gypsums. The conglomerate is worked still with good results. D. HONEYMAN

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Japanese Mirrors

A SHORT time ago a friend showed me a curious effect, which I had previously heard of, but had never seen. The ladies of Japan use, in making their toilet, a small round mirror about $\frac{1}{2}$ to $\frac{3}{4}$ inch in thickness, made of a kind of speculum metal, brightly polished and coated with mercury. At the back there are usually various devices, Japanese or Chinese written characters, badges, &c., standing in strong relief, and brightly polished like the front surface. Now if the direct rays of the sun are allowed to fall upon the front of the mirror and are then reflected on to a screen, in a great many cases, though not in all, the figures at the back will appear to shine through the substance of the mirror as bright lines upon a moderately bright ground.

I have since tried several mirrors as sold in the shops, and in most cases the appearance described has been observed with more or less distinctness.

I have been unable to find a satisfactory explanation of this fact, but on considering the mode of manufacture I was led to suppose that the pressure to which the mirror was subjected during polishing, and which is greatest on the parts in relief, was concerned in the production of the figures. On putting this to the test by rubbing the back of the mirror with a blunt pointed instrument, and permitting the rays of the sun to be reflected from the front surface, a bright line appeared in the image corresponding to the position of the part rubbed. This experiment is quite easy to repeat, a scratch with a knife or with any other hard body is sufficient. It would seem as if the pressure upon the back during polishing caused some change in the reflecting surface corresponding to the raised parts whereby the amount of light reflected was greater; or supposing that of the light which falls upon the surface, a part is absorbed and the rest reflected, those parts corresponding to the raised portions on the back are altered by the pressure in such a way that less is absorbed, and therefore a bright image appears. This, of course, is not an explanation of the phenomenon, but I put it forward as perhaps indicating the direction in which a true explanation may be looked for.

The following account of the manufacture of the Japanese mirrors is taken from a paper by Dr. Geerts, read before the Asiatic Society of Japan, and appearing in their *Transactions* for 1875-76, p. 39 :—

"For preparing the mould, which consists of two halves, put together with their concave surfaces, the workman first powders a kind of rough plastic clay, and mixes this with levigated powder of a blackish "tuff-stone" and a little charcoal powder and water, till the paste is plastic and suitable for being moulded. It is then roughly formed by the aid of a wooden frame into square or round cakes; the surface of the latter is covered with a levi-

gated half-liquid mixture of powdered "chamotte" (old crucibles which have served for melting bronze or copper) and water. Thus well prepared, the blackish paste in the frame receives the concave designs by the aid of woodcuts, cut in relief. The two halves of the mould are put together in the frame and dried. Several of these flat moulds are then placed in a melting box made of clay and "chamotte." This box has on the top an opening, into which the liquid bronze is poured, after it has been melted in small fire-proof clay crucibles. The liquid metal naturally fills all openings inside the box, and consequently also the cavities of the moulds. For mirrors of first quality the following metal mixture is used in one of the largest mirror foundries in Kiôto :—

Lead	5 parts.
Tin	15 "
Copper	80 "

100

For mirrors of inferior quality is taken—

Lead	10 parts.
Natural sulphide of lead and antimony	10 "
Copper	80 "

100

"After being cooled the melting-box and moulds are crushed and the mirrors taken away. These are then cut, scoured, and filed until the mirror is roughly finished. They are then first polished with a polishing powder called *to-no-ki*, which consists of the levigated powder of a soft kind of whetstone (*to-ishi*) found in Yamato and many other places. Secondly, the mirrors are polished with a piece of charcoal and water, the charcoal of the wood, *ho-no-ki* (*Magnolia hypoleuca*) being preferred as the best for this purpose. When the surface of the mirror is well polished it is covered with a layer of mercury amalgam, consisting of quicksilver, tin, and a little lead. The amalgam is rubbed vigorously with a piece of soft leather, which manipulation must be continued for a long time until the excess of mercury is expelled and the mirror has got a fine, bright, reflecting surface."

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THE DECENNIAL PERIOD OF MAGNETIC VARIATIONS, AND OF SUN-SPOT FREQUENCY

A CENTURY and half ago Graham discovered that the north end of a magnetic needle moved from morning till afternoon towards the west, returning thereafter to its most easterly position in the morning again. Van Swinden, who, half a century later, studied this phenomenon during several years, occupied himself greatly with the deviations from the diurnal law. One of these, the occurrence of the greatest westerly position before noon or after 4 P.M., he found to happen most frequently in 1776, the number of times increasing from 1772, and diminishing from the year of maximum till 1780. He then asked the question whether there was not a period of *eight years*. Van Swinden's results were greatly affected by imperfections of his instrument, and we can only consider that the excess of irregular days in 1776 was probably chiefly due to real causes.

Though several series of magnetic observations were made during the eighteenth century, and two series early in this (those of Beaufoy and Arago), yet, as far as I can discover, Kaemtz seems (in 1836) to have been the first to remark that the mean value of the diurnal oscillation of the magnetic needle was not constant, but varied from year to year: this conclusion he founded on Cassini's observations, which gave the mean oscillation 9°71 in 1784, and 15°10 in 1787. The illustrious Gauss drew more distinct attention to the fact, for, in studying the observations made at Göttingen in the years 1834 to 1837, he pointed out that the mean diurnal oscillation for each month in the second year was greater than that for the corresponding month of the first year; and that a similar increase was to be found in the third year compared with

the second. This increase Gauss did not think could go on long, and he predicted that by continuing the observations for several years, an oscillation in the mean value would present itself. It is not a little curious that in discussing the Göttingen observations for the next three years, Dr. Goldschmidt should have failed to remark that the maximum was attained in 1837, and that thereafter the mean diurnal oscillation was diminishing. This was reserved for Dr. Lamont, the distinguished astronomer of Munich, who, in the end of 1845, by adding the mean oscillations obtained from his own observations in 1842-1845, to those already found for the preceding years at Göttingen, was able to state that the minimum was then attained, but that a longer series of observations was required, in order to determine the law of the oscillation.

It was only in the end of 1851, when the maximum oscillation (which occurred in 1848-49) was decidedly past, and the mean oscillation had again begun to diminish in value, that Dr. Lamont published his conclusion that the diurnal oscillation of magnetic declination (as well as of magnetic force) obeyed a law whose mean duration was nearly 10½ years. For the determination of this mean he employed the epoch of maximum oscillations shown by Cassini's observations in 1787 (already noticed by Kaemtz), and he assumed that there were six periods from that date till 1849.

Schwabe had previously, from his persevering observations of the number of spots on the sun's surface, arrived at the conclusion that these obeyed a decennial law, so that the number was a maximum in 1828, 1837, and 1848, while it was a minimum in 1833 and 1843. The agreement of the epochs, 1843 and 1848, with those of minimum and maximum magnetic disturbance deduced by Sir E. Sabine from the observations made in the colonial observatories, was at once remarked by him, as well as that of Lamont's epochs with those of Schwabe.

This coincidence was also immediately afterwards, and quite independently, brought to public notice by Dr. Wolf, of Bern (now of Zurich), and M. Gautier, of Geneva. It is, however, with the important labours of the former of these philosophers that we are most concerned. Dr. Wolf began at once a systematic search for observations of sun-spots, and examined hundreds of volumes printed and in manuscript, dating from the first discovery of the existence of spots on the sun's surface. All the observations thus accumulated he has endeavoured to connect and to reduce to a common unit; and from the numbers thus obtained he has concluded that the sun-spot period, as well as that of the magnetic variations, occupies on the average 11½ years.

One great cause of the difference between the results of the Munich and Zurich astronomers is to be found in the interval 1787 to 1818. According to the former, three periods *ought* to have occurred in this interval; according to the latter, only one maximum happened, *in fact*, between the two of 1787 and 1818. Dr. Wolf has concluded, from the magnetic observations of Gilpin (1786-1806), that a minimum of the diurnal oscillation of the magnetic needle occurred in 1796, and a maximum in 1803, and these epochs he has supported by the observations of the numbers of sun-spots, as well as of those of the aurora borealis, a phenomenon known to be associated with magnetic disturbance, and to have the same epochs of frequency. On the other hand, Dr. Lamont has maintained that Gilpin's observations are without value, as his needle was supported on a steel pivot, and sometimes did not move freely; he has also objected to the observations of sun-spot frequency made during the time in question, that they were made rarely, without any common system, and by few observers, some having at times seen no spots when others saw many.

If we could assume with the astronomer of Munich that Gilpin's observations and those of sun-spot and auroral frequency made at the same time are worthless,

all our knowledge of the epochs of magnetic oscillations since 1818, and of sun-spot frequency since 1826, would induce us to conclude that there were really three periods during the thirty-one years 1787-1818. If, however, any value can be given to the observations during that interval, it is not allowable to assume that the durations of the periods have always been the same, the more especially that we know the period has varied in length from eight to twelve years within the last half century. That some value is due to observations of three different phenomena has been allowed by most writers, and Dr. Wolf's period of 11½ years has, in consequence, been accepted by many of the most eminent men of science who have had occasion to allude to the subject.

Having had to study this question in connection with the results of observations made during twenty-three years at Trevandrum, I have examined with care the magnetic observations of the last and the present century, determined the exact times for which the yearly mean diurnal oscillation of the magnetic needle was a maximum or minimum, and have arrived at the following conclusions:—¹

1st. That there are not sufficient grounds for rejecting the observations of Gilpin, which appear to be in general trustworthy as regards the change of mean position of the needle from year to year, and of the diurnal range from winter to summer.

2nd. That these observations should, according to the mean law, show a maximum near 1797, and another should have occurred near 1807. I have found that they do indicate a maximum in the former year; and though another maximum appears in 1803, that there are grounds for believing the maximum may really have occurred after 1806, when Gilpin's series terminated.

It has to be stated, however, that the maximum shown by Gilpin's observations in 1797 is very small; that the whole interval between the preceding and following minimum is not six years; and that no such short period and small maximum have been observed during the last half century. Since, however, the shortness of the period and the smallness of the maximum are both confirmed by the observations known to us of the frequency of sun-spots and of the aurora borealis, I can only conclude, in conformity with the facts, that both these were real phenomena, which may yet be repeated and aid in the determination of the cause of the decennial period. The mean duration of the period at which I arrive is therefore almost exactly that which Dr. Lamont had previously obtained, or 10·45 years.

For this result the facts have been taken as they present themselves; since it would be difficult to conclude that the observers of all the three phenomena could have erred in the same way during nearly twenty years. In addition to this, after a careful study of Dr. Wolf's sun-spot numbers, I find it impossible to accept his period of 11½ years. How ill the facts satisfy this result may be shown by two comparisons in which the epochs accepted by the Zurich astronomer are employed.

Thus a maximum of the magnetic oscillation occurred in 1787 by the observations of Cassini and Gilpin; this epoch has been confirmed nearly by Dr. Wolf's sun-spot numbers, and by Prof. Loomis for the auroral frequency. We have then the last observed maximum 1870·9, about which there can be no doubt. In the interval between these two maxima there were, according to Dr. Wolf, only seven periods, consequently we have—

$$\frac{1870\cdot9 - 1787\cdot3}{7} = \frac{83\cdot6}{7} = 11\cdot94 \text{ years,}$$

a period which differs as much from his mean period as that does from Dr. Lamont's. If on the other hand we take one of Dr. Wolf's sun-spot epochs about eighty years

¹ See "On the Decennial Period," &c., *Trans. Roy. Soc. Edin.*, xxvii., pp. 563-594.

before 1787, and employ the number of periods he has himself given for the interval, we find—

$$\frac{1787.3 - 1705.5}{8} = \frac{81.8}{8} = 10.23 \text{ years.}$$

If, then, we commence with the epoch of 1787 and compare it with any epoch of maximum since, we shall always find for the mean duration at the least 11.9 years according to Dr. Wolf; and if we compare it with any of the epochs given by him upwards of eighty years before, we shall never find a greater mean than 10.75 years, and this result includes an interval of 172 years before 1787, with all the uncertainty of the earlier epochs. This great difference of more than one year in the mean duration, as derived from eighty-four years after 1787, and eighty-two to 172 years before, disappears to a great extent if we admit three periods between 1787 and 1818.

It has been already remarked that the duration of a period is not constant, but varies within certain limits. The question naturally presents itself—Does this variation follow any law, or is it accidental, increasing one year and diminishing the next? The number of periods for which we have the epochs of maxima and minima of the diurnal oscillation of the magnetic needle accurately determined, is not sufficient for any very sure reply. At the same time the results I have obtained indicate a period of nearly forty-two years for the repetition of the variations in question; and if this conclusion is confirmed by next maximum, that should occur in the year 1879. It may also be pointed out that according to the law of forty-two years a maximum should have occurred in $1818 - 42 = 1776$. Now this year, according to Dr. Wolf, was a year of minimum. The variation of his sun-spot numbers for that period, it appears to me, is not sufficient to give his conclusion much weight; while, on the other hand, Van Swinden's result, which it is extremely probable was a consequence of the decennial law, gives 1776 for the year of maximum; and that it was so is further supported by the magnetic observations of Cotte, at Montmorency. The exceptional period about 1797 shows, however, that any definite conclusion from observations during the last sixty years may be impossible, since causes of variation exist with which we are insufficiently acquainted as yet.

When we compare the mean range of the diurnal oscillation of the needle for the year in which it is a maximum with that for the year of minimum at any station, we find that the ratio of the two is very nearly constant for places so widely separated as Toronto, Dublin, Trevandrum, and Hobarton. I have also found that the *law* of the diurnal movement is the same in the year for which the range is least, and in that for which it is greatest. This shows that it is the same cause which is acting, the variation being one of intensity only. Since few or no sun-spots are visible in the years of minimum range, we perceive that the sun-spots happen only when the intensity of the force producing the magnetic variations exceeds a given value. It also appears that considerable variations in the amount of magnetic disturbance may exist near the equator when there are few or no sun-spots; and, on the other hand, that the spotted surface of the sun may be a maximum, and no corresponding increase of the magnetic oscillations be visible. The latter are, however, exceptional cases, since increases of sun-spots and of magnetic movements occur frequently near the same time; the increase of the one, however, bears no constant proportion to that of the other.

It has been already stated that the ratio of the diurnal oscillation of the needle in the year of maximum to that in the year of minimum is very nearly constant for places very widely separated from each other; there are, however, slight variations in the ratio shown at some places; thus, although it is nearly the same at Toronto, Dublin, Trevandrum, and Hobarton (1.55), it is slightly greater for Munich

and Lisbon (1.71). This is probably due to the action of disturbances which are known to obey local laws. I have also found for Trevandrum, nearly on the magnetic equator, that the disturbances, or the deviations of the magnetic needle from the mean position, do not show exactly the same epochs of maximum and minimum in the decennial period when different hours are considered. Thus, though the cause is cosmic, the actions appear to be influenced, though but slightly, by circumstances of locality.

When we seek for the cause of the decennial period, we are met at first by the three phenomena which obey this law: the magnetic variations, the sun-spots, and the aurora borealis. The connection between the first and third is so marked, that if a magnetic disturbance commences during the day in a high latitude, it is quite certain that the aurora will be seen as soon as the disappearance of sunlight permits. This is a fact I have verified during several years' observations in the south of Scotland. Both these phenomena are results of electrical motions. It did not seem improbable then that the solar spots might be connected with disturbances of electrical equilibrium, and that these might be due to the different electrical states of the sun and of the planets.

We do not know, however, of any planet with a period of ten and a half years, nor of any combination of planetary positions which would produce such a period. My own researches have failed in connecting the variations of the sun's spotted surface with the time of revolution of any planet by a law which holds for different decennial periods. This fact, however, does not disprove a planetary action. We are unacquainted with the nature of the medium through which the electrical actions producing the magnetic variations are conveyed. Physicists seek to reduce the phenomena of nature to the fewest possible factors: many then have been induced to believe that electrical and magnetical actions are conveyed by the same ethereal medium which we believe transmits heat and light. The facts do not appear to be easily explained by such a hypothesis; thus I have found that certain electrical actions of the sun producing marked diminutions of the earth's magnetic force happen exactly at successive intervals of twenty-six days; when one point or meridian of the sun returns to the same position relatively to the earth; this action, similar to that of a beam of light reflected from a revolving mirror, which illuminates a particular point only at the same part of its revolution, has no resemblance to that of light and heat, which are propagated equally in all directions.

If, then, we can suppose that the electrical medium is disposed unsymmetrically around the sun, that the disposition and extension varies, it is obvious that the supposed planetary actions would also vary, and might be quite different for different parts of their orbits, in different decennial periods. This suggestion may explain why I have not been able to find a law remaining the same in the different periods; and it is not opposed to the conclusions of Messrs. De la Rue, Stewart, and Loewy, who have found very remarkable relations between certain positions of the planets and the amount of the sun's spotted surface during a single decennial period.

Any hypothesis which seeks to explain the mode of production of the sun-spots (by cyclones or otherwise) must also explain why the causes become insufficient for their production every ten and a half years. M. Faye, the distinguished French astronomer, considers that the prime cause of sun-spots is to be found in the excess of heat radiated; so that the spots are the symptoms of a dying sun; that we have in fact here a phenomenon like the flickering of an expiring lamp which may have a periodical character. Such a hypothesis will scarcely satisfy the demands of science, but we must evidently wait for more facts before any satisfactory theory can be proposed.

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